

Now, by analogy with the solution for the free spherical material point, we can suppose that the quantity $f/(df/dn)$ is zero at the points occupied by the light quanta at a given instant. Then the velocity of a quant in passing by a point M will be, for example, 5 :

$$v_M = \left(- \frac{\partial f / \partial t}{\partial f / \partial n} \right)_M = c^2 \frac{\partial \phi}{\partial n}. \quad (6)$$

The motion of the quanta is permanent and $-c^2\phi$ plays the part of a velocity potential.

But, during the motion, the number of quanta remains constant and along a very thin tube of trajectories, we must have :

$$\rho \sigma v = \text{constant}, \quad (7)$$

where ρ is the mean density of light quanta in the wave and σ the section of the tube.

Hence, we conclude that :

$$\frac{1}{\rho} \frac{\partial \rho}{\partial n} + \frac{1}{v} \frac{\partial v}{\partial n} + \frac{1}{\sigma} \frac{\partial \sigma}{\partial n} = 0. \quad (8)$$

But infinitesimal geometry teaches us that the last term of the first member is equal to twice the mean curvature of the phase surface. Thus, equation (8) gives easily :

$$\frac{1}{\rho} \frac{\partial \rho}{\partial n} = - \frac{\Delta \phi}{\partial \phi / \partial n}, \quad (9)$$

and by comparing (9) and (4) we see that :

$$\rho = c \cdot a^2. \quad (10)$$

The density of light quanta is to be taken proportional to the classical intensity. In the dark fringes of the classical theory, the density of quanta will be zero, but in a bright fringe a great number of quanta will pass. Now, the motion being permanent, this explanation of the experimental facts will still be available if the light is very weak (Taylor's experiment) ; we have only to define the density of quanta by a time average instead of a space average.

LOUIS DE BROGLIE.

Paris, August 27.

Science and Psychical Research.

I HAVE read the editorial note appended to the letters on this subject published in NATURE of September 11, and desiring to keep within the limits that you wish to be observed in this discussion, I have tried strictly to confine myself in the following remarks to replying to allegations against myself made by Sir Arthur Conan Doyle in that issue, as also to the specific points raised by Dr. Tillyard in his rejoinder to my letter on this subject published in NATURE of August 28.

Sir Arthur Conan Doyle states that my account of the incidents connected with the Combermere photograph, published in the issue of August 28 "is both inaccurate and misleading." It will, I think, be sufficient to direct attention to one only of Sir Arthur Conan Doyle's statements, to show whether he is, or I am, the more accurate person. After telling how I challenged him to publish, in the *Morning Post*, the ghost photograph alongside a photograph of the peer taken in life, Sir Arthur Conan Doyle goes on to say (italics are mine) — "I at once sent up my photograph *without any suggestion whatever that it would not reproduce*. That statement is a pure invention on the part of Mr. Campbell Swinton."

In reply to this, may I quote the opening sentence of Sir Arthur Conan Doyle's letter to the Editor of the *Morning Post*, published in that paper on April 23 ? It is as follows :

"I beg to enclose the Combermere photograph. I

am advised that it will not reproduce, but you will be the best judge of that."

These two entirely contradictory statements, both from the pen of Sir Arthur Conan Doyle, show how little reliance can be placed upon the accuracy of what he writes, and I therefore do not propose to make any further reference to the remarks contained in his letter to NATURE except to say that they consist of a tissue of misrepresentation, together with a number of statements which are no more accurate than the one quoted above.

With regard to Dr. Tillyard's rejoinder, I should like, first of all, to say how much I appreciate the spirit in which he has accepted what may have seemed to him my somewhat provocative criticisms. I must, however, further criticise what he now says.

Dr. Tillyard complains that I do not distinguish between spiritualism and psychical research ; but, so far as I can see, the only distinction between the two is that the second includes the first, while I may add that, though the heading under which it appeared was "Science and Psychical Research," Dr. Tillyard's article purported to be a review of a "History of Spiritualism."

Nowhere have I ever suggested that the medium is one of the experimenters, as Dr. Tillyard surmises. On the other hand, I cannot agree with him that the medium is a mere instrument, such as a microscope or spectroscope, for, quite apart from the question of free will, which is probably an illusion, due to the same portion of the brain being concerned in determining both our wishes and our actions, human beings have consciousness and motives, which are things possessed by no man-made instrument or mechanism.

Dr. Tillyard states, "If a medium is found to be fraudulent, then the genuine psychical researcher will not proceed with him, but will endeavour to find a more trustworthy one." But this, anyway, was not the method adopted by Crookes, who, I suppose Dr. Tillyard will agree, was one of the greatest of psychical researchers. If we are to believe the authorised life of Sir William Crookes, written by Dr. Fournier d'Albe, "Crookes does not seem to have taken up the medium (Miss Cook) seriously until after she had been exposed by a Mr. Volckman, who seized 'Katie King' (supposed to be a spirit) and found himself holding the medium (Miss Cook) dressed up."

Furthermore, Miss Cook, who had by then married and become Mrs. Corner, was again exposed by seizure, when masquerading as a spirit, by Sir George Sitwell, the well-known baronet. A detailed account of this exposure will be found under the heading "Capture of a Spirit" in the *Times* of Jan. 12, 1880, while in the *Times* for Jan. 15 following, there is a letter from the secretary of the British Association of Spiritualists (on whose premises the seance was held), on behalf of the council of that body, stating that Sir George Sitwell's account of what occurred was substantially correct. Sir George Sitwell quite recently told me that this complete exposure, which had wide publicity, made so great a sensation that it nearly wrecked the whole spiritualistic movement of that time ; so Crookes must have known all about it. Yet, a few years later, as recorded in Sir Arthur Conan Doyle's history, we find Crookes giving an unqualified testimonial as to the bona fide mediumship of this twice-convicted impostor.

All scientific men hold Crookes in the highest veneration both as a physicist and as a chemist, but what can they think of his judgment in respect to spiritualistic matters, having regard to what is disclosed above ?

Dr. Tillyard mentions the names of Crookes and of four other scientific men who studied spiritualistic

phenomena, and then asks me whether I can name a single one of these who did not become convinced of their genuineness. Surely this is a case of begging the question, as these are the particular five who did become convinced; but what about the others? What about Faraday, Tyndall, Sir David Brewster, and Dr. Carpenter, among those who are dead, and Prof. R. W. Wood of the U.S.A., Sir E. Ray Lankester, and Sir Bryan Donkin among those who are living? All these and many others have, I believe, made sufficiently serious investigations into the subject, though naturally, having come to the conclusion that there was nothing genuine in the phenomena warranting further research, they did not publish so much as other no more eminent, though perhaps more credulous investigators.

It may also be mentioned that, in 1908, a committee, including such eminent photographic experts as R. Child Bayley, F. J. Mortimer, and E. Sanger-Shepherd, though assisted by such a well-known spiritualist as Mr. A. P. Sinnett and others, failed to secure proof that spirit photography is possible.

Dr. Tillyard suggests that I should visit the National Laboratory for Psychical Research, but I must confess that I am not attracted by its name, which with its suggestion of parallelism with the National Physical Laboratory, seems to me to be *suggestio falsi*. I am informed that it is a purely private concern, with nothing national about it whatever. Apart from this, however, in my opinion thermographic phenomena in connexion with mediums are more a matter for a physiologist than for a physicist. That emotional disturbances affect the temperature of the body is, I think, fairly well known, and there does not seem to me to be any reason for dragging in such supernormal and incredible phenomena as the production of ectoplasm and such like in order to explain what are only commonplace matters. But then, all psychical researchers seem to delight in the maxim *omne ignotum pro magnifico*.

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September 10.

The Structure of the Continents.

As all the continental discussions of the observations of near earthquakes have been carried out by graphical methods, and as I could not satisfy myself as to the precision obtainable by these methods, I have recently carried out a rediscussion of the principal series of data by the method of least squares. These refer to the Kulpa valley earthquake of 1909, the Wurtemberg one of 1911, the Tauern earthquake of 1923, and the Oppau explosion. The results indicate very definitely that there is an upper layer that transmits compressional waves with a velocity of 5.6 km./sec. (though a velocity of 5.4 km./sec. would fit the Oppau explosion slightly better) and a lower one where the velocity is 7.8 km./sec. In addition, the Tauern earthquake gave rise to a wave with a velocity of 6.2 km./sec., which must have travelled in an intermediate layer. The probable error of all these velocities does not exceed 0.1 km./sec. The result for the upper layer corresponds to that found for granite by E. D. Williamson and L. H. Adams. The recent work of L. H. Adams and R. E. Gibson gives a velocity of 6.4 km./sec. in basaltic glass, and of 8.4 km./sec. in dunite, at ordinary temperatures and at pressures corresponding to depths of some tens of kilometres. If we allow for the higher temperatures within the crust, the basaltic layer below the granite may be in a glassy state, as Daly has suggested, and the lower layer may well be dunite. The evidence indicates

that there is no further sudden change to a depth of about 1200 km.

The times of arrival of all the waves were linear functions of the epicentral distance; the consistency of the observations was great enough to give good determinations of the gradients of these functions, and hence of the velocities, but it was not enough to establish any departure from linearity. Hence there was no material for a determination of the depths of the foci or of the variation of velocity with depth in the various layers. By combining the results for near quakes with those for distant ones, however, it was possible to estimate the rate of increase of velocity with depth in the lower layer.

The observations permit a rough determination of the depths of the granitic and basaltic layers. The former may be about 12 km., the latter about 20 km., but both are subject to an accidental error of about 4 km. In addition there is a possibility of systematic error. Uncertainty as to the depth of focus may allow the thickness of the granitic layer to be doubled. On the other hand, the movement on the seismogram due to the indirect waves starts more gradually than that due to the direct one, and this may cause a slight delay in their measured time of arrival, especially as most of the observations seem to have been made on instruments recording on smoked paper. On this ground the depths determined may require some reduction.

I think, therefore, that determinations of the depths of the layers by means of near earthquakes are not more reliable than those based on the earth's thermal state, isostatic balance between continents and oceans, and the group-velocities of surface waves. All of these are affected by uncertainty concerning the thickness of the basaltic layer, but the uncertainties of the method based on the compressional waves from near earthquakes appear more serious. The results, taken as a whole, are as consistent as can be expected; a thickness of 10 to 15 km. for each layer would be within the range of uncertainty of every method.

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Curved Path of Wireless Waves.

IN a recent number of the *Proceedings of the Royal Society* (Series A, vol. 111, N.S. 757) there appears under the title of "Discussion on the Electrical State of the Upper Air," a paper giving the views of several of the authorities who have contributed to the examination of this subject.

The discussion really turns on the question as to why wireless waves follow the curvature of the earth instead of spreading into space.

Heaviside supposed that this was due to a hypothetical conducting layer of the atmosphere existing at a great altitude above the earth's surface, which would act as a reflecting barrier and would compel the wave to remain within the envelope formed by it. Only one contributor to the discussion referred to refraction and diffraction as possible causes.

If it were assumed that the speed of long waves is the same as that of ordinary light, and depends in the same way on the density of the air, then refraction would account for, roughly, one-tenth of the observed deflection. I believe, however, that there are no direct measures of the velocity of long waves, and there is no sufficient knowledge of the nature of the 'ether' or of its relation to ponderable matter to allow of any certain, or even probable, theoretical deductions on this point.